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Talent Developed: Conversations with Masters of the Arts and Sciences

Rena F. Subotnik

Joshua Lederberg: Scientific Risk Taker and Innovator

Dr. Lederberg won the 1958 Nobel prize in physiology and medicine for research demonstrating that bacteria, like other organisms, possess a genetic system that could be employed as models for genetic experimentation. Among Dr. Lederberg's other revolutionary accomplishments is the development of a heuristic computer program called DENDRAL. This powerful program has allowed scientists to efficiently explore research possibilities derived from interesting molecular configurations.

Subotnik: Please tell our readers about the nature of your work.

Lederberg: The center of my professional life is my laboratory which I've reconstituted during the last two-and-a-half years. Most geneticists are interested in differences in primary DNA structure—the mutant gene vs. the normal gene. But I start out with a given DNA and ask how it can fold itself in different fashions making it vulnerable to different kinds of genetic damage and change. Predatory molecules lying about in the cell, or even radiation for that matter, will have a different likelihood of causing genetic damage in the unravelled regions as opposed to those that are tightly wound up. It's a complicated interaction.

RS: How come you chose to explore this specific field?

JL: I started my research stimulated by Avery's discovery about DNA as the transforming molecule. I began bacterial genetics in order to provide an experimental vehicle for following up on that observation.

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The immediate question was how do you get the right experimental material from bacterial genetics to explore the chemistry of the gene? In the 70s, at a point when the questions were very clear, the chemical technology to pursue this investigation was out of my grasp. The technology has finally matured to the point where I can use it in a modest lab.

I've always believed in going after high-stake, high-risk kinds of projects. Since I played a rather significant role in establishing the existing system of thinking in the area, I thought it was my job to be the revisionist in another incarnation. The topic also happens to be very timely in terms of newly available technology.

RS: How did you develop your sense of what is a good research question? Is it something that you were born with, or did someone help you learn this important skill?

JL: I start out with an intuition, or with some observed behavior on my part. Then I step back and say, "Josh, what are you really doing? What principles are you following? Is this a special case of some general principle? Let's discover what the principle is and what kind of case can be made around that."

I'll give you another example. I'm often invited to do this or that, and I'll sometimes be tempted to accept. But before I do, I'll step back and say, "Look, you're being a pawn. Somebody invited you to do something and you agreed to it. If you're that interested in following it up, why don't you take a more aggressive posture and figure out what you would do within that framework, if you had complete freedom of action." So that's another revealed behavior.

RS: Did you get guidance with these heuristics along the way?

JL: I don't think that I was ever told that heuristic, but it may have been demonstrated to me in various ways. The person who would have had the most influence in that regard was Francis Ryan, my undergraduate mentor at Columbia University. He knew a lot, a lot more than I did. But I had an ease of jumping to the next layer of abstraction that I've not seen widely displayed by anybody else, so I don't think I learned it any place that I could identify.

RS: Do you feel that the post docs or the scientists that are coming to work with you have this skill highly developed?

JL: I think they do in varying measures, but they are not self-conscious about it. Part of my job is to get them to another layer of organized insight so that they can call on this resource and not just wait for it to happen spontaneously.

RS: How do you do that? By example?

JL: Mostly by example. Every now and then we will talk about heuristics. A related one, but not at the same level of abstraction, is that of relentless combinatorics. Once you have identified what some of the variables might be that could be included in experimental design, or influence the outcome of an experiment, the abstraction would be to say, "These are some of the variables. What are the others? What are the ones we haven't talked about yet? Do we have some way of exhausting the space of possible variables? What happens if we look at all of them in any imaginable combination?" We can't always do that, so we say, "What rule of reason can I apply that would give me an efficient and likely successful reduction in the search space that still involves as wide a set of these components as possible." I don't know my own or anybody else's mental processes well enough, but it's the only heuristic I know how to implement on the computer, allowing for the process to be mechanized.

Back to problem choice. There are elements of taste. Some experiments just seem like fun, seem like they are very easy to do and will answer a question that appears to be interesting at the time. That's a fairly powerful motive. Every now and then you step back and think about a research strategy, again generalizing from those instances, scanning for what's the most revolutionary thing that might be a possible outcome, reviewing your techniques and resources to see if you have any chance of getting there. There are a lot of exciting questions that are unreachable and a lot of easy things that are sort of trivial, so trying to find some balance between them is the strategy. At one stage, about 30 or 40 years ago, I wrote down what I thought were the major unresolved questions in biology. One by one, they've all become very nearly resolved. So, in a way, by the end of my lifetime, I may have actually seen the resolution of all the big questions that I started with in science.

RS: What is the process by which you recruit talented people to work with you?

Joshua Lederberg. (News and Publications Service, Stanford University, Stanford, CA 94305)

JL: The network of personal connection is probably the most important way, although there have been slightly more systematic vehicles. When I set up my lab, I had a very sharply focused research project in mind. I knew that it would have very high appeal to very few people. I put ads in a number of places—a couple of the major journals and *The Scientist*—mentioning what the program was and said, "Don't bother to apply unless you can say something meaningful about the topic in question." I got about a dozen replies. If it had been an undifferentiated ad for a post doc, experience tells that I would have gotten about 300 responses. All 12 applications were pertinent, and half of the candidates were of sufficient quality that I would have felt happy to have taken almost any of them. The three more senior people that I have in my lab right now were an outcome of that circulation.

RS: What was the nature of your interview. What were you looking for when you met them?

JL: I wanted to know that they had a background of technical competence. I needed very experienced people because I was setting up a lab *de novo*. They were going to have to be pretty much on their own that first year, getting only the most general guidance from me. My job was to raise the money, set the general direction of the lab, and see to it that the lab could get organized. They would have to take on a lot of the detail. That's quite a daunting task, particularly because they would be taking about a year out of their most productive period of scientific activity to set the lab up. But it would also give them experience with the sort of autonomy that might not ordinarily be available at this early point in their career. So it was a consciously arrived-at contract. Their independent thinking was the most important consideration, but it had to be coupled with proven competence to actually do the lab work. And I got what I looked for.

RS: What, in your opinion, is the role of luck or chance in the development of high-level science talent?

JL: The case I know most about is my own personal experience, and there the greatest element of luck was my finding Francis Ryan. I, no doubt, would have sought somebody out as a mentor, but Francis was a very special person. I know that nobody else in the Department of Biology at Columbia appealed to me the way he did, and that's already telling you something. Above all, he gave me a sense of discipline.

My mind was a riot of ideas. I needed to know how to shape those ideas into a specific research program, how to focus my attention on a few things I wanted to do. I learned that very directly from him. I adored him, and I would do anything he suggested or wanted. To have his esteem was my most important aspiration during that time. He knew how to design experiments and would be quite rigorous in what one needed to do to put all the elements together. He used Socratic discourse and the dialectical method extensively. Rarely would he tell me, "No, you've got to do it this way." He allowed me a sense of self-discovery, the mark of a wonderful teacher.

My insight on how to choose an undergraduate college was very, very limited. I had no idea that there was going to be Francis Ryan at Columbia. And I had no idea of what to expect by way of relationships with professors. I knew there would be people who were very knowledgeable and to whom I could ask questions. I also knew that they did a lot of research there, and I hoped that I might somehow connect with it. Otherwise, the process of college selection was very, very vague. I didn't know anyone in the profession that I could talk to or get any guidance from.

RS: What about peers? When you were school age, did you have a group of peers you could talk to?

JL: I was lonely in grade school. Very lonely. In high school, and this is at Stuyvesant High School (specialized science high school in New York City), there were three or four kids my own age that I could feel comfortable with sharing ideas and experiences. None of them was directed on the same career path that I was, but at least they helped to assuage that isolation I experienced when I was younger. Stuyvesant was all boys at that time, and it was difficult to meet any women that I could relate to on that kind of plane. That was very unfortunate. I certainly felt so at the time.

At Columbia the students were not as rigorously selected as they were at Stuyvesant, and I didn't have many effective peers among the other undergraduates. I did have the lab, however. I related much more to Francis than I did to any of the other students. My peers ended up being the graduate students in that department. The age disparity required some social adjustment on my part, but they came to accept me without reservation.

RS: How did you deal with your loneliness in elementary school?

JL: I think part of my intensity of preoccupation with academic studies was that I didn't have the distraction of social interactions.

RS: Were you harassed at all?

JL: No. In fact, I have no recollection of being harassed in school. I very lately had a reunion with someone who was my classmate in third, fourth, and fifth grade. I asked her what she remembered from that period and she said, "Well, you were a figure of awe. We really respected you very much. We knew that you had problems in your social relations, but we were willing to give you all kinds of allowances because of that admiration." Abbey also said, "We were kind of baffled; we just didn't know what to talk to you about." And she affirmed that most of the teachers were equally sympathetic, and equally baffled, with what to do with me.

RS: Were you in any kind of special class?

JL: No. I skipped grades, that was it.

RS: How many grades?

JL: I graduated high school in January of 1941 when I was 15½. So it was about two years, I guess. Columbia wouldn't admit me until I was 16, so I had a little hiatus there. I spent it working in a lab.

RS: And even at Stuyvesant you only found three people to talk with at a really high intellectual level?

JL: I have to put it fairly bluntly. Although I had a very special affinity for those three, I don't think I had intellectual peers at Stuyvesant either.

RS: Did you ever feel that you were abnormal because of the intellectual distance that existed between you and the world around you? Or did you experience enough support outside of school?

JL: My teachers were wonderfully supportive. I wasn't harassed by other kids in school. But they didn't know what to do with me, and I did not, at early ages, have easy social relations with either sex, especially not girls. It wasn't until I connected with Francis and the graduate students (men and women) at Columbia that I felt there really were peers who were equal, had both the confidence and the substance to put up a good argument, and would sometimes show I was wrong.

RS: Would you say, then, that you lived in a family and school system that valued your being, but couldn't necessarily deal with you at the same level?

JL: The important thing was that they knew when to leave me alone. That was my contract. In public school, the contract was quite explicit. When I got bored, I would try to show up the teacher when she didn't do the math just right. It almost brings tears to my eyes to think of the compassion and understanding which Sadie Gold, my teacher, showed me. Mrs. Gold asked me to stay after class and said, "Josh, we know that you're brilliant; you ought to know that we know it. But let's make a contract. I have a job to do; I've got these other kids who don't have your gifts, and they've got to learn. Let's be partners, and the prize includes your own development. If you get bored, do your own work. I won't bother you, but don't disrupt the class." The sense that she was approaching me as an equal was quite extraordinary.

So I had all the passive elements of support. But nobody took me in charge, gave me a sense of what to do. I was allowed to follow my own resources and got lots of reinforcement. You know, "You're an unusual person, you have gifts, you ought to do something with them. We're not sure what to do to help you, but you're doing just fine on your own." And that's more or less the way it worked out.

In high school I was an anomaly. With rare exception, I knew more than the teacher about the subjects that I was learning, certainly in the sciences. If I ever sensed that a teacher knew more than I did, I repaired the difference. I would just hit the books and go to work and master it. That was not true in every subject. I remember having a thrilling course in civics. I didn't take any more political science in college or whatever, but I think it gave me a very solid grounding. The teacher was someone who really understood his subject and dealt with it at a very effective level. But within the sciences, with all the vaunted advantages of Stuyvesant, it was not the teacher's knowledge of science that was critical. Perhaps I take that too much for granted. They were sympathetic to science, they were encouraging about it as a career, but my peers were more important to me in that respect than my teachers. But remember, I was a freak, so I don't judge by my own experience what they ought to be and do in relation to other students.

RS: Do you think that people who are intellectually developed far beyond the mainstream should be left to their own devices? Don't they need teachers too?

JL: They need guidance. The most efficient way to teach is to teach people how to learn as much as possible on their own. I don't know how far down you can carry this, but most of my education involved my own reading. I could have used more guidance on what to read, how to structure what was going on, even an occasional exam now and then as a way of pacing what was happening. If I had met Francis six years earlier, that would have been enormously helpful without necessarily taking a lot of his time. I'm trying to organize mentorships, including something not as costly or intense as having kids spending hours and hours working in the lab, but some form of organized guidance.

RS: What advice do you have about the training of teachers to work with talented students in science?

JL: The part that I know most about has to do with their cognitive depth, their knowledge of the content, and that's difficult to monitor with science moving as quickly as it does. Whatever they have learned in school tends to be obsolete pretty quickly. So, I think continuing education is probably the major issue. But I also think that boards of education need to understand that if you deliver eight hours a day of teaching in the classroom, you don't have time for professional development. On the other hand, it's not automatic that, if a teacher is relieved of classroom duties, he or she will spend time on professional development. Somehow you have to find ways of structuring both access to and taking advantage of continuing education. There ought to be a level of awareness and educational enhancement that's between the graduate textbook level on the one hand and the popular books on the other. I suppose *Scientific American* isn't too far different from that.

RS: How did your children describe their most gifted teachers?

JL: More in terms of inspiration than content. They made the subject interesting, they were able to deal with questions, they managed discourse in the classroom and kept students involved, and they knew how to relate the subject matter to other aspects of the world. The usual. But the kids in the school were not in a position to make critical judgments about what a teacher does and doesn't know. They only know when it's a flop; so if it isn't, they sort of take content knowledge for granted.

RS: Let's tie this in to your interest in helping and guiding young scientists. What age groups do you think would be most receptive and most positively affected by this mentoring?

JL: It's at the high school level that I'm hoping to find some way of setting up a mentoring system where students who might have an interest in science can get some well-informed grounding about what to do to achieve that aim or to understand better what their relation to that aim might be: What to do about their studies, how to allocate their time between what they have to do in class and their own reading, or what to think of in terms of college aspirations.

You raise an interesting question about what age groups to deal with. I don't know whether 11th or 12th grade might be late in the differentiation of students' interests or whether I could do much with the younger kids. Fairly casual contacts are not likely to be too productive. I'm not sure how I would come across to an eight-year-old child who didn't know me.

RS: According to the research that I've read, ninth grade is the point that differentiates which students are in or out of the science pipeline.

JL: If ninth grade is critical, then that's what we ought to focus our attention on. I think, even at a younger age, a graybeard like me might still be able to have a meaningful contact. I have a sense that it just takes longer to cultivate an interest and to get to know them than it would for the somewhat older student.

RS: I think, graybeard or not, if you approach them with respect, in the same way that you described your elementary school teacher speaking to you, the students would respond. What would be the logistical nature of your idea?

JL: One-on-one is what the core of it has to be. But I need a lot of help on this point. I only have vague ideas on how it might be structured, but I thought there could be a clearing office, maybe at The New York Academy of Sciences that would have lists of volunteers who would be willing to be available and would then also, mostly through the schools, identify the teachers who would be interested in cooperating. Some teachers will find it threatening to their own monopoly of relationship. Perhaps there could be a way in which professional scientists would make some group appearance, but then indicate that the students who are interested, and they might need a little egging on by their teachers to overcome their bashfulness, might get some guidance, advice, and instruction. And it's not necessarily going to be signing up for a lab experience, which is the immediate presumption as soon as I mention this at all.

RS: What are your views on the value of contests like the Westinghouse Science Talent Search?

JL: As a way of eliciting interest on the part of students who come in with marginal involvement, it's probably all to the good. I'm not sure it's the best way for committed students to spend their time if they are willing to put their noses to the grindstone. You can't learn too much by way of basic mathematics, physics, and chemistry, no matter what else you are going to do, especially if you are going to go into biology. It sounds like an anomaly, but if a young person is really interested in biology, he or she should postpone doing biology and make that the last and not the first thing explored in depth. Unless we get more students who have had intense mathematical preparation, we're not going to get the biological breakthroughs. I think there are similar things to be said about chemistry and physics.

There are inappropriate expectations for being able to do any really creative and original work in the lab. I'm not saying that that's always the case, but if you are going to have to come out with a poster to present in the Talent Search, the implication is that this is "all mine." Laboratory experience provides the best learning when it helps to habituate you to most experiments not working the first time, to then going back to fix it up. If getting that kind of discipline can be done in a supportive way, and it isn't too much of a challenge to the student's self-esteem, then the lab experience could be considered part of an apprenticeship. That part is good.

RS: What are the alternatives to working in a lab where you have access to equipment and expertise?

JL: I think book studies are the best alternatives, but you may need both inspiration and guidance to stick to that. To this day, I'm still torn between learning what I can from reading the literature as compared to the very slow and inefficient process of my own personal discovery in the lab. In half an hour, I can get the distillation of 10 years of somebody else's work by reading it. There's always the need to keep the balance between the two.

RS: Unfortunately, you can't do a review of the literature for a Westinghouse project.

JL: That is very unfortunate. For one thing, knowing how to do it is a very important skill. It's just as important as knowing how to do the lab work. I think reviewing the literature is underemphasized; one of the things I would do in counseling students is to try to find something that they are curious enough about to really want to know all that can be known about it from reading. The chance that they will find out the same amount or make a major advance by doing a lab experiment is very low, and less if they don't have their grounding in the reading to know what the platform for that research would be. My druthers would be much more emphasis on the research library as compared to the research laboratory.

RS: How about introducing high school students to the concepts of research design, like validity, reliability, etc.?

JL: I think criticizing other experiments is more efficient than doing it on your own. I mean, it may be more dramatic when you've been through it yourself, but there again, it's a two-stage matter. I think first there should be a critical analysis of published experiments, and then one could be in a better position for self-criticism of your own design. I feel it's partly a matter of staging. The junior year of college is in some ways the more appropriate time to think about organized research, once you've got four more years of the literature. Then the likelihood of being able to do a serious research project is much greater. It doesn't have to be all or none, but I certainly would find more ways to put emphasis on the library. For a student to jump straight into the contemporary literature is asking a lot, but I think with some counseling and support, a lot can be done in that direction.

RS: In earlier days, more students participated in the Westinghouse Science Talent Search. Currently, students seem to need more enticements to think of themselves as scientists. Can you elaborate on how science seems different today than it was when you were a novice?

JL: I doubt there is a baseline to make any generational comparison. I judge what another generation thinks just from what I see about science in the popular culture. In a sense, there's no such thing as "science" anymore. There are a lot of different "sciences," and sub-sub-specialties get to be the dominant elements. You don't expect anybody today to be conversant in both cosmology and molecular biology. If you are going to be in science, you have to come up very quickly with a differentiated interest. To get to those sub-specialties, there's a longer and more daunting course of training. I may have been the last of any generation who could do a significant experiment as an undergraduate in a new field. I would ask myself where would there be opportunities like that today. I'd be inclined to guess that I'd need another four, five, or six years of specialized training to be capable of doing anything at the frontiers today. So string and sealing wax are less and less effective as scientific instruments and a larger and larger proportion of the work that could be done at that level has already been done and exploited. Those are the cognitive elements.

There are also what I'd call ideological and ethical considerations, more ambiguities about how science relates to human affairs. The Bomb has been the crowning technological achievement of modern science, and that's elicited Frankensteinian images.

RS: David Feldman, a developmental psychologist, has proposed a "theory of coincidence" as related to prodigies. He believes that one of the major variables affecting prodigious behavior is the stage of development of the field in which a prodigy appears. This theory speaks very well to what you are saying. If a field is too highly developed, it's hard to nurture a prodigy because it's not feasible for a child or a young person to have achieved enough experience or knowledge to make an impact. If you can't do really frontier-level work until you are a post-doc, then the field becomes less attractive.

JL: It's also part of what you have to counsel the youngster on. On the one hand, you have to have realistic expectations; on the other hand, you cannot be so dampened by realism that you destroy the motive to go into it in the first place.

RS: Do you see any budding areas where a young person could be a "giant" in biology?

JL: It's a question I've asked myself many, many times, and others have as well. All the areas I know about are very well populated right now. I don't know if it's the poverty of my imagination or if it's really true that the habitation of science has become uniformly dense. Most of the domains that are not thoroughly developed are borderline areas of applications rather than fundamental theory. The only place that I could expect some significant breakthrough is in new approaches that are going on in the mathematization of biology. It would be something very different from the logical mathematics being done right now, but until we develop better ways of codifying our present knowledge, we are enmired in this ever-increasing fragmentation difficulty. I have a program in computer science affiliated with my lab program that is intended to kind of nibble away at those sorts of issues, but it's going to take more than we are doing to make any significant breakthroughs in that field. And it may be unfeasible. This is what I label as Leibnitz's dream, and I'm not sure how seriously to take the enterprise. Leibnitz's dream, the codification of knowledge, is the area that I see as waiting for a new conceptual breakthrough; and it's one of the reasons I've been interested in computer science for a number of years. We've done some work that provides steps in that direction, but it doesn't begin to be the major leap that I'm looking for.

The Human Genome project, for example, is a very well-articulated set of challenges that is exciting and important at every level from basic science on to invaluable biomedical applications that are almost inexhaustible. It's the opening up of a frontier with a fairly well-delineated geography. The chief limitation with exploration these days is funds. Now, it may seem a little dull to say I'm going to look at gene number 47,744; but the fact is, once you get into it, every one of them has a lot of wonders to tell you and that's been the experience of a lot of very capable and successful young scientists. What's harder to get is a synoptic overview of the whole enterprise.

RS: How about the question of lifestyle? That seems to be an issue for the children of the 80s. Unless they can be convinced that the intellectual pleasures of science surpass the low income and huge time commitment, lifestyle issues remain very pressing.

JL: Well, there's a certain circularity in that. If you're not involved in science, you're not going to be interested in it. I think most scientists say that the lifestyle they prefer is one of involvement. The preoccupation and so on are part of the fun.

RS: How about the time spent seeking grants?

JL: That's really coming to a head just now, and I think that probably is a deterrent at this stage. If you look at students' career choices at the college level, they have been, until very, very recently, heading in droves into law and business; and you read that an MBA is worth $100,000 salary right off the bat. If you have to be grubbing around for grants for years to make it in science, I'm sure that that's a deterrent. On the other hand, that argument can be overdone. I think any youngster with real talent doesn't have to worry. I see thousands of scientists who are in fairly happy situations with their lives, doing the work they want to do. In many respects their income, relative to other occupations, has improved somewhat, although probably not relative to business. It would be interesting to get some facts on this question.

Traditionally, science has been a career that has, in the United States, offered some mobility. Scientists have risen, very dramatically in many cases, from what their parents' economic status had been. Jews, in particular, had few good alternatives in earlier times. Science and medicine seemed like the obvious things to do because you were going to run into more immediate ceilings in other professions. With medicine, you had problems getting into medical school; but once you were there, you were pretty secure after that point. I think, for Jews, the more or less complete disappearance of restrictions on mobility may be one of the reasons that less of them are going into science than used to be the case.

I have a feeling that the moral issues are even more important than the economic ones. Your ninth graders, I don't think, have heard a lot about grant seeking, but they've heard plenty about what scientific monsters can do.

RS: What I have found in my conversations with young people talented in science at the middle school and high school level is a concern about using animals in research.

JL: Well, it's certainly an issue. It's dissuaded my daughter from going into biology. She just couldn't bear the thought of dissection. She understood why it had to be done, but she couldn't do it herself. Biology education is going to have to continue to take into account this reaction on the part of students. Some students get into biochemistry through chemistry so they can avoid the dissection components of their academic preparation.

But there are other ideological issues. When you start out with a generation like mine that had, in general, much deeper religious convictions, I think a commitment to science was in some measure a displacement, a sublimation if you like, of that. Religious issues generally don't seem to count for very much right now; so I think there is, to that degree, correspondingly less impetus for science to play a role in people's lives.

RS: Theoretical physicists design logical, mathematical ways to prove theories. Is there something like that going on in biology?

JL: Very little. I've indulged in a bit of it myself, mostly with the view of the theoretical framework for my own research. But every now and then there's an opportunity to do it on a larger scale. My main contribution of that kind was in the elaboration of the theory of immunology. They call it "Clonal Selection Theory." In fact, I had organized how to think about the problem, but I relied on a false premise. So I walked right up to an articulation of the theory and then, in effect, rejected it, but for the wrong reasons. It's unique in my experience, and rare in general biological experience, that a theoretical paper could have that much impact. Evidence has come in to bolster every piece of it, so I don't want in any way to minimize the importance of experiment, but it was a case where clarification of theoretical issues was absolutely central.

RS: Why do you think that physics and biology have developed so differently?

JL: You're dealing with a much more complex set of entities, and generalizations are never going to be as rigorous. Even my theory of antibody formation has lots of soft edges to it by comparison with any physical theory. Extend that one step further to behavioral science, and I don't have to tell you the further problems you have trying to make generalizations on multitudes of organisms.

RS: Today people seem to be more able to live without putting order in the world around them.

JL: It's not a question that's presented to children in an a-religious world, and so there's less motive for what I'd call a counter-religion. Some people would say that science led to the deflation of religion which, in turn, reduced some of the motive to think about doing science.

RS: Do you think that science could be made more attractive to spiritually oriented students by focusing on some of the more aesthetic or the cosmological sides to it?

JL: I think that already happens by way of self-selection in careers, but the fragmentation of science goes against that. If you had the feeling that you might have been among the "giants" who really changed our world view at a very fundamental level, you would feel that you were dealing with some very important eschatological issues. The expectation of being able to do that in today's fragmented world is so low. If you are going to be a discoverer of a new particle, you'd be one of a team of 300 people. So I think that must be a major demotivating factor. We're back to the issues of complexity and fragmentation again. I feel fortunate. I've been able to keep a perspective about science which is much broader than almost any of the other people I know, but I do feel like a dinosaur in that respect.